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STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF MICHIGAN.

Contributed by W. B. PILLSBURY.

I. THE FLUCTUATIONS OF THE ATTENTION IN SOME OF THEIR PSYCHOLOGICAL RELATIONS.

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Since the first consideration of the fact of periodically varying intensity in the perception of minimal stimuli, no one has felt that any final word has been spoken. The matter was considered important as bearing on the ultimate theory of the attention during the historic discussion, beginning with Lange and ending with Pace, but afterward it was mentioned in the literature as one of the general facts of the attention compatible with almost any theory, all question of causality being left open. This state of affairs has a degree of justification if we remember that fluctuation is by no means the greatest phenomenon referred to when we speak of attention, a term covering a wide range of facts and of exceedingly indefinite connotation. If any one is so rash as to overreach in his inference, he will soon find his theory going to pieces before an advance along some seemingly unrelated line. So the cautious attitude is not to be entirely discouraged or distrusted. At the same time it is evident that any investigation of this kind must have as its ultimate purpose the elucidation of the most complex and hitherto most baffling problem in psychology, viz., that of the essential nature of the attention. Some are content to rest in general terms, as, for example, that attention is the activity of consciousness as a whole, basing their position upon some simple formula, as the law of relativity, without recognizing the fact that such a formula, perfectly sane as far as it goes, still goes only a little way toward elucidating the processes involved, until the limits and manner of its application are fully determined. The discussion referred to pivoted on a question arising from the analogy, useful for some purposes, but often misleading, which compares consciousness to the field of vision and makes a separation of the perceiving agent from the object perceived. Do the so-called fluctuations of the attention belong

to consciousness or to the contents of consciousness? Very few to-day would insist that this is a paramount question or that its answer would give a final solution to the problem of the attention. We may say that the attention is some form of interaction of the conscious contents, but various perplexing questions immediately arise. Is it not a mere figure of speech to speak of "contents" with a conjoined function of interaction? Is this interaction a process of reinforcement or inhibition or both? What physiological evidence can be brought in support of the view?

Experimental demonstrations, especially of a physiological kind, have come in very slowly in a field where they are most urgently needed. Still certain important additions to our knowledge of the physiology of the nervous system, coupled with modified views of the attention, which demand a basis in the facts of nerve function, render an orientation of the question unavoidable.

The facts to be dealt with are simple and familiar. If attention is given to any just noticeable stimulus, as the ticking of a watch held at the proper distance from the ear, the gray rings of the Masson disk, or slight electrical stimulation of the skin, a periodic variation between perceptibility and imperceptibility is noticed. These are the so-called fluctuations of the attention.

I.

Since the reviews of the literature bearing on the question are fragmentary and unequal, it will be of advantage to give a brief description of what has been done in this field. The phenomenon, already noticed by physiologists and physicists, was first systematically dealt with by Urbantschitsch in two investigations.¹

In the first of these he found that when a clock is placed at such a distance from the ear that the ticking is just noticeable, a variation in the clearness of the sound, ranging from the distinct appearance of the separate strokes to entire disappearance, is perceptible, the transition being in some cases gradual, in others sudden. The same result was reached when the ear was closed and the sound transmitted through the bones of the head. On the basis of these facts, he concluded that the fluctuations have their seat in the acoustic nerve, which being subjected to continuous stimulation, soon becomes fatigued and recovers itself only after a certain period has elapsed. In the

¹ Ueber eine Eigenthümlichkeit der Schallempfindungen geringster Intensität. Centralblatt f. d. med. Wissensch, 1875. Ueber subjective Schwankungen der Intensität akustischer Empfindungen, Pflüger's Archiv. Bd. 27.

second investigation, Urbantschitsch experimented with the two ears at once and with subjects having defects of hearing, and reached the additional conclusion that the periods for the two ears do not coincide but alternate. The noise is noticed first on one side, then it seems to pass through the head and appears on the other side. In similar manner and with like results, Urbantschitsch investigated the other senses. If two points are placed on the skin at the "limen of twoness," the periods fall first above, then below, the limen. If the points are further apart, the fluctuation is between the two just as in the case of the two ears. If the two index-fingers are placed in hot water, the pain sensation is felt first in one, then in the other. He found that the same rule holds if two points of any sensory surface are affected by their specific stimulus.

The view of Urbantschitsch that the phenomenon depends on the periodic exhaustion and recovery of the sensory nerves, admittedly without support from what was known of nerve physiology, found an opponent in Nicolai Lange who investigated¹ the variations applying to them the name "fluctuations of the attention." His view was that the fluctuations were of central origin and depended in general upon the reinforcing function of the apperceptive activity. After establishing the periods as extremely short he proceeded to his crucial experiment which was to compare the periods found by applying minimal stimuli to disparate senses at the same time. It was found that the periods did not fall together but were separated by a definite interval. On the basis of this experiment, very questionable in itself because of the difficulty of attending simultaneously to disparate minimal stimuli, Lange drew the immediate conclusion that the fluctuations did not depend upon fatigue of the sensory nerves, as Urbantschitsch had thought, but must be referred back to the unitary activity of apperception. The first or negative part of this conclusion, as Eckener indicates, has a certain ground if we grant the accuracy of the experiment. The second part, as Münsterberg tells us with justification, throws the whole problem back upon an activity which, as Lange conceived it, lies partly or entirely within the region of the transcendental. Lange then proceeded to support his position with experiments on the familiar illusion of the steps and broken wall, finding that the fluctuations between the two fall approximately within the same time limits as those connected with minimal stimulation. Reasoning from the assumed fact that the change in the illusion is due to the varying power of reinforcement of the memory images of steps

¹ Beiträge zur Theorie der sinnlichen Aufmerksamkeit und der aktiven Apperception, Phil. Studien IV, pp. 390, ff.

and wall in the process of apperception, Lange concludes that some such varying memory images must lie at the basis of the fluctuations in the case of minimal stimulation. That this is a pure inference, unsupported by direct observation, may be seen by constant attention to any just noticeable stimulus, as the gray rings of the Masson disk, which during their disappearance leave nothing perceptible but the already present white background, showing that it is a case of appearance and disappearance and not of alternation. In general, one is inclined to favor the criticism which Münsterberg¹ passed upon Lange, "*die Resultate seiner Experimente sind durch ihre überraschende Eleganz, durch ihre unerwartete Konsistenz und Sicherheit, durch ihre leichte Verchmelzbarkeit mit metaphysischen Ansichten, einerseits geradezu prädisponiert zu wissenschaftlichen Dogmen zu werden, andererseits in hohem Mass den Verdacht nahe legend, dass irgendwelche Fehlerquellen übersehen worden sind.*"

The next investigation was that of Münsterberg² who took a position in opposition to that of Lange, asserting on the basis of his experiments that the phenomenon in question is to be referred to the fatigue and recovery of the accommodation muscles in the sense organs. This is the peripheral theory which is combated by nearly all the succeeding writers on the question. Münsterberg's investigation was limited to the sense of sight, on which he performed a series of elaborately varied experiments. His reason for choosing this sense department was, "*Den Lichtreiz können wir unter sehr viel mannigfaltigeren Bedingungen darbieten als den Schallreiz. . . . Wichtiger aber ist, dass wir das Auge auf beliebige Punkte richten und bewegen können.*" The method of experimentation was to fixate the gray rings of the Masson disk and record the fluctuations in series upon a white kymograph drum by means of a tambour and pencil, in this way improving upon the method of Lange, who measured single fluctuations upon the chronoscope. The general average of the normal fluctuations was first established as 6.9 seconds. The experiments were then varied by the use of a "*prismatische Lorgnette*," by which the field of vision was moved slightly to the side, requiring a quick movement of the eyes in order to keep the fixation continuous. After some practice the subject could make the necessary movement so quickly that the interruption was scarcely perceptible. In case the prisms were held continuously before the eyes naturally no very great change in the fluctuations would enter. But when the glass was inter-

¹ Beiträge zur experimentellen Psychologie, Heft II, p. 78.

² Die Schwankungen der Aufmerksamkeit, Beiträge II, pp. 69, ff.

posed at intervals of two seconds, it was found that the fluctuations could be lengthened to 11 to 14 seconds. The average was 12.3 with an average variation of 3.1 seconds. In the next series, a sound was made by an assistant every second, which caused the subject to close the eyelids quickly for a moment, making a scarcely noticeable interruption in the fixation. "A decrease to entire vanishing never took place." A quick voluntary closing of the eyes every two seconds produced the same result. The next variation was by the interposition of a gray covering which for a short interval completely hid the disk from view. "The effect is now entirely different, . . . the vanishing appears much oftener than by normal, uninterrupted fixation." The average length of the fluctuations was 5.8 as against the normal 6.9 seconds. The same general results were reached by making other variations, such as the use of indirect vision, and moving the whole apparatus slowly in different directions. In the last series of experiments, Münsterberg investigated the connection between breathing and the fluctuations. When the respiration was in short gasps a distinct shortening of the fluctuations was noticed; a lengthened respiration gave a corresponding lengthening of the fluctuations. In the latter case there was often a direct correspondence between inspiration and vanishing, but not seldom the two proceeded with entire irregularity.

Münsterberg's theoretical conclusions will concern us here for only a moment as they will be noticed later on. Realizing that the intervention of a transcendental function of consciousness never suffices for a scientific explanation, he decided that the variation must lie in the region of "contents," *i. e.*, must have a peripheral origin. He accounts for the process in this way. The gray rings of the disk, standing out only in the slightest degree from the background, require exact accommodation and fixation. Any cause, either artificial or of the nature of fatigue, which produces a change in the tension of the muscles, necessarily renders impossible for the time being the perception of the rings.

In opposition to the view of Münsterberg appeared a series of articles, the first of which was by Eckener.¹ This investigation is important as bringing the apperceptive factor in the process strictly within the lines of scientific explanation. Münsterberg had practically admitted the transcendental element in making the separation between consciousness and the conscious contents, the first being in the process under consideration a fixed will to attend, a complex of numerous motives,

¹ *Untersuchungen über die Schwankungen der Auffassung minimaler Sinnesreize.* Hugo Eckener, *Phil. Studien*, VIII, p. 343.

etc., the second being the act of perception itself. Eckener in a rather rigid criticism shows the impossibility of this distinction.

What Eckener does, beyond the work of Münsterberg, is to extend the method of investigation to other sense-departments, and on the basis of these results and a careful introspective analysis of the conditions involved, draw the general conclusion that all the causal factors are of a central nature. In particular, he points out the close connection between the ease with which the memory-image of a sensation is kept in clear consciousness and the fluctuation of the sensation itself. The conclusion from this is that the general psychophysical condition which connects the memory-image with the actual process of stimulation must vary in some way before the fluctuations can appear. The causes of such a variation are not far to seek. They lie partly within the nature of consciousness itself as an organization of dynamic not static elements, partly in the activity of other sensations claiming a share of the attention. In other words, the reason for the fluctuations lies in the familiar phenomenon of distraction. The criticism we will here pass upon Eckener is that he states the conditions of the problem without giving a real solution. Suppose we grant that the process is of a central nature, how much nearer are we to an intelligible understanding of its real nature? The term *appception*, as he understands it, has no clearly definable laws, and, until we can determine them, it is absurd to apply it for purposes of scientific explanation.

Pace, in a companion article¹ to that of Eckener, describes an experimental test of Münsterberg's conclusion that the fluctuations depend upon variations in visual accommodation. Working only with the Masson disk, he first establishes the averages for the normal vision of his subjects, then paralyzes the ciliary muscles by the use of atropin, and finds that, with the power of accommodation entirely lacking, the fluctuations proceed with only a slight variation from the normal. The obvious conclusion is that the essential conditions are central. The slight variation would indicate some kind of reciprocal action between center and sense organ, but as to the nature of this he gives no opinion.

In the investigation of Marbe,² carried on without knowledge of what was being done by Eckener and Pace, the position of Münsterberg is attacked from another side. "Die Theorie Münsterberg's . . . ist nur haltbar wenn drei Voraussetzungen

¹Zur Frage der Schwankungen der Aufmerksamkeit. Phil. Stud. VIII, p. 388.

²Die Schwankungen der Gesichtsempfindungen. Phil. Stud. VIII, p. 615.

erfüllt sind: wenn nämlich erstens die Schwankungen nur bei ebenmerklichen Reizen eintreten; wenn zweitens die Reize, damit sie überhaupt sichtbar werden, exakte Accommodation und Fixation erfordern, wenn drittens die Schwankungen nur bei dunkeln Punkten auf hellen Grund stattfinden." On this basis, a few simple variations in the method of experimentation are sufficient to overthrow the opponent's position. What Marbe really does is to determine the fact that when the intensity of the stimulus is varied within the very narrow possible limits, in the case of visual sensations by changing the degree of brightness or the distance from the eye, a ratio is found between the degree of variation and the length of the fluctuations. With a stimulus of greater intensity or nearer the eye, the periods of disappearance are shorter. We shall refer to this again.

In the investigation of Lehmann,¹ the theories of the former writers are rejected as one-sided, and an attempt is made to analyze some of the physiological factors in the process. Following a suggestion made but rejected as improbable by Münsterberg, he studies by a simple experimental arrangement the relation of breathing to the fluctuation. In the case of slight electrical stimulation, it is found immediately that the curves representing the respiration and the course of fluctuations, are in large measure coincident. This is not so evident in the case of sound or light stimulation, but, by plotting the averages of a large number of experiments on co-ordinates divided to represent the different stages of a respiration, the general result is determined that the maxima of the fluctuations fall near the highest point of the inspiration. To account for the irregularity, Lehmann brings in other factors, in the case of sound, the related memory-image determined by Eckener, in the case of light, both the memory-image and Münsterberg's variation in accommodation. As to the precise way in which the respiration affects the fluctuations, we are left in some doubt. ". . . die Reactionen sind am häufigsten in der Nähe des Inspirationsmaximums. Hier ist eben der Blutdruck am grössten, und von diesem Zustand muss angenommen werden, dass er für die psychophysische Arbeit des Gehirns günstig sei. Wir wissen ja, dass das Blut, während der Arbeit irgend eines Organes, demselber reichlicher zufließt." No one will dispute the fact that any organ in activity, by reflex excitation of the vaso-motor center, receives a larger supply of blood. This is eminently true of the brain. But that the activity of the muscles of respiration should cause

¹ Ueber die Beziehung zwischen Athmung und Aufmerksamkeit. Phil. Stud. IX, p. 66.

a greater flow of blood to the brain does not appear from this process of reasoning.

The valuable investigation of W. Heinrich¹ regarding the influence of central processes upon the activity of the visual sense organ, contains two results of importance to our consideration.

(a) "Wird die Aufmerksamkeit nicht-optischen Eindrücken zugewendet, so wird das Auge akkommodationslos, es kann sogar eine noch stärkere Abflachung der Linse eintreten, wie beim Fernsehen."

(b) "Wird die Aufmerksamkeit von den optischen Eindrücken abgewendet, so ändert sich die Konvergenz der Augenachsen. Diese nähern sich der Parallelstellung."

Heinrich proceeds without critical examination to accept the peripheral explanation of the fluctuations, not recognizing the fact that, on Münsterberg's ground, so soon as the central factor is admitted, this interpretation is no longer possible.

This review of the literature affords us a basis for distinguishing three views regarding the fluctuations: that they have a central cause, that they have a peripheral cause, and that they have a purely physiological cause. Each of these is open to certain criticisms.

(1) The school of Wundt finds itself in the unfortunate position of being committed to the interests of a definite theory. This theory, notwithstanding the fact of its great utility, is still so general that facts referred to it are not really explained, and, like all general theories, is easily disturbed by any apparently antagonistic fact. The consequence is that its supporters approach every question with a certain degree of pre-conception, which shows itself in the present case. (a) The variation must be in the clearness and not in the intensity of the sensation. (b) The falling away from clear consciousness must be due to interference and inhibition by other conscious elements. (c) The explanation must be predominantly psychological. This last is a basis of a very valid objection. If the phenomenon is to be viewed purely from the standpoint of apperception, there is nothing to determine its periodicity but the nature of the conscious elements involved. These are admittedly of very unequal importance in consciousness. On the other hand, the fluctuations, in spite of their variations, show a significant degree of regularity. This would predispose one immediately to look for some rhythmical physiological process as a basis of explanation.

The peripheral theory of Münsterberg, on the other hand, has the advantage of being at first sight very simple and plausible. But to grant that the fluctuations depend entirely upon

¹ Die Aufmerksamkeit und die Funktion der Sinnesorgane. Zeitschrift für Psych. u. Phys. d. S., IX, p. 342.

fatigue in the accommodation muscles of the sense organ, is to grant the previous assumption that this mechanism is relatively independent of the central factors. No such isolation seems possible. First, muscular fatigue, after the researches of Mosso and Lombard, must be interpreted as exhaustion of the inner-vating center. If it is replied that this is a purely reflex process, then, secondly, we may adduce the well known fact of central control of reflexes. This was definitely proved in the case of the eye by the investigation of Heinrich noted above. The primary impulse must come from the central organs. Further the theory may be questioned on the basis of fact, as the results of Pace's experiments show. Our own work on Dr. O., mentioned below, enters in as a definite disproof.

A theory like that of Lehmann, besides being very indefinite in itself, describes processes which have no psychical counterpart, so, however useful it may be in other directions, does not lend itself to the explanation of a psychological phenomenon.

II.

Our experimental work took, as its point of departure, two facts derived from former investigations: (a) that the weight of evidence lay decidedly on the side of a central causal process of some kind; (b) that the general theories of the attention and apperception required physiological supplementation before they could give an adequate explanation of the phenomenon. This gave a determinate direction to the work. It was necessary,

1. To make an accurate, detailed study of the fluctuations in order to determine any characteristics manifested in their actual process of occurrence.

2. To study the connection between these characteristics and any physiological processes that might be found to influence them.

The experiments were begun in the fall of 1898, and continued for the greater part of two years. The following persons acted as subjects for all or part of the experiments: Prof. Pillsbury (P), Miss Earhart (E), Messrs. Vought (V), Kirtland (K), Stevens (St), Dr. Oliver (O), and the writer (S). All of these except O. had had considerable training in psychological methods of work. None of them except Prof. Pillsbury and the writer had any knowledge as to the purpose of the investigation. Each subject was given several days' training before his results were treated as trustworthy.¹

¹ Perhaps no kind of reactions are at first more indefinite than those in this field. The object obtained by the preliminary training was the reflex registration of *all* the fluctuations and the exact boundaries of each. The best test of adequate preparation was the subject's own feeling of satisfaction with his reactions.

In order to carry out the first of the objects indicated above, it was necessary to obtain the fluctuations in series of suitable length. For this purpose a long horizontal drum was used, covered as usual in registration experiments with smoked paper. A small stand, bearing the Marey tambours and electrical markers, was moved along the drum by a threaded rod, turning uniformly with the revolutions of the drum. In this way a continuous series, lasting as long as ten minutes, could be secured. The tambour recording the fluctuations, was connected by a rubber tube with another tambour provided with a pressure-key which was operated by the finger of the subject. Time was recorded by a Jacquet-Verdin clock, indicating fifths of a second. The minimal stimuli used in nearly all the experiments were the gray rings of the Masson disk, as they were found to be most suitable for continuous series, as well as for furnishing the most unambiguous results. The disk used was of white cardboard and 26 cm. in diameter. Along one of the radii was a series of carefully outlined black spots, each of which was 4 mm. square. The distance of the spots from each other was likewise 4 mm. As the daylight was found to be exceedingly variable for the work, the reflected light of an incandescent lamp, carefully shaded from other parts of the room, was allowed to fall upon the disk. The distance of the subject from the disk was kept uniformly at one and one-half meters.

1. NORMAL FLUCTUATIONS.

The average time-lengths for the different subjects and different series were found to vary within such wide limits, that our original intention to make the individual fluctuations as occurring in a series the object of study, soon found sufficient justification. The time values were plotted directly on cross-section paper, a millimeter to the fifth of a second, those representing the visibility of the stimulus being above, those representing the non-visibility, below the horizontal axis. Distance was, of course, allowed on this axis for the lengths of both, reduced, however, to seconds. The general results will be evident from the following curve. The numbers indicate full seconds.

This curve is taken from the very earliest series, but the same characteristics are shown throughout. The characteristics are evident.

(1) The curves representing the visibility and the non-visibility of the stimulus do not remain constant but vary in approximately definite periods of two kinds, the first from 10 to 15 seconds, the second from 60 to 80 seconds in length.

(2) These variations seem, in a measure, independent of the number of fluctuations.

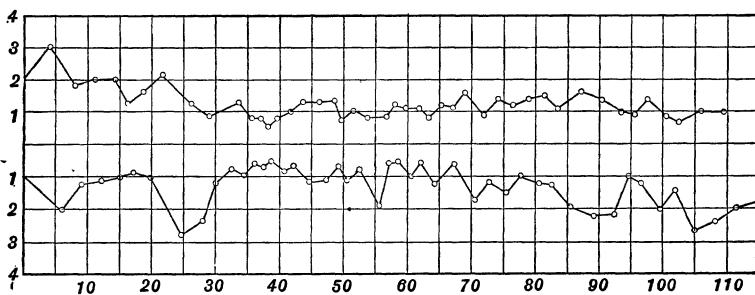


FIG. 1. SUBJECT P.

(3) The periods of visibility and non-visibility vary their length with reference to each other not in inverse but in direct proportion.

(4) In general, as the curves approach each other, that is, as the periods both of visibility and non-visibility shorten, the former seems to be relatively greater in length than the latter.

These generalizations are based upon the results of about forty series, of which the short section reproduced is considered representative. While these data afford very meager evidence for any kind of general conclusion, still they give certain suggestions which it may be pertinent to indicate.

(a) The fact that we find in addition to the primary fluctuations a secondary and a tertiary period regularly recurrent and largely independent of the number of fluctuations, would suggest that we are dealing with a composite process, showing the influence of several distinct physiological rhythms.

(b) The fact that a period of visibility is attended by a proportionally long period of non-visibility suggests the erroneousness of the commonly accepted view that the length of time a stimulus is effective in consciousness is a direct measure of the efficiency of the attention. If we can assume that the ratio of attention-efficiency is the ratio of the preponderance of the time-lengths of visibility, the converse seems to be the case. The most effective attention is attended by short and therefore rapidly recurring fluctuations. This position is sustained by the investigation of Taylor carried on in the Michigan Laboratory the past year. The experiments were sufficient at any rate to confirm our original opinion that the fluctuations are of central origin but incapable of explanation on a purely psychological basis, moreover, that the investigation of the physiological processes involved would be an analysis of the groundwork of the attention.

2. EFFECT OF VOLUNTARY EFFORT.

It is a well known fact that the efficiency of voluntary effort in any part of the body is increased if attended by strain in other parts. The only possible explanation of this seems to be that the efficient discharge of a group of motor cells is in direct proportion to the extent of area excited. Or, in other words, the impulse from the motor cell is not only transmitted along the regular path for the innervation of the muscle, but is diffused to the cells of other motor parts of the cortex. Can a like diffusion extend to sensory cells? If the general law is established, it will be a comparatively easy matter to show how the centers controlling the rhythmical functions of the body exert the influence on the course of visual fluctuations already indicated.

To study this effect the same arrangement was continued as before, except that another tambour was added connected by a rubber tube with a Verdin dynamograph. In the later experiments a Cattell spring ergograph provided with a tambour was substituted for the dynamograph. The course of fluctuations was taken normally as before for the first half of the series, then, at a word from the experimenter, pressure was exerted upon the instrument by the left hand of the subject. Of course the strain could not be continued for any great length of time, so the recording pointer gradually came to its first position. One fact that may be noted in this connection is that the pointer invariably fell during a period of non-visibility, the effort being always sustained during the period of visibility. The general results of the method will be evident from the following figure.

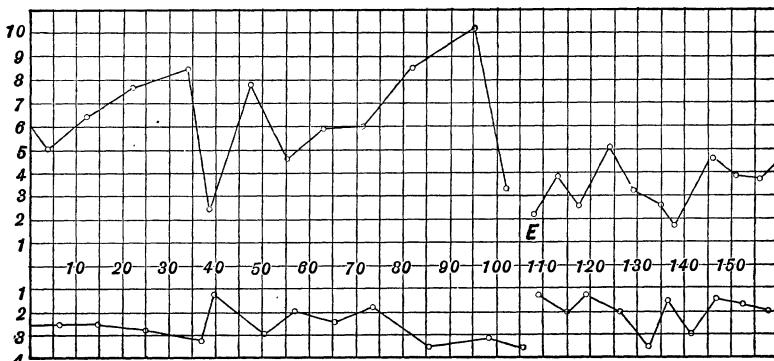


FIG. 2. MADE ON SUBJECT K.

The figures indicate full seconds. At point marked E., strain upon ergograph is begun.

The fact which this figure is intended to illustrate is obvious. The exertion of pressure upon the ergograph by the subject is followed by an immediate and distinct shortening of the periods both of visibility and non-visibility. An additional fact, not so clear from the figure but established by all the averages made by the writer, is the relative increase in the periods of visibility over those of non-visibility. Adding the remainder of the ergograph series to the figure shown, and averaging, we get the following Table.

TABLE I.
Subject K.

	N.	AV. F.	AV. V.	AV. UV.	RATIO OF V TO UV.
Normal	13	45	31.9	13.1	2.43
With Ergo.	28	33.4	24	9.4	2.55

The Table gives the complete series of which Fig. 3 is a part. The following Table shows the same results for another subject.

TABLE II.
Subject V.

	N.	AV. F.	AV. V.	AV. UV.	RATIO OF V TO UV.
Normal	21	29	16.5	12.5	1.32
With Ergo.	28	23.4	13.9	9.5	1.46

N represents the number of fluctuations in the series, *F*, the value of the whole fluctuation, *V* and *VV*, the periods of visibility and non-visibility respectively. The last column shows the increased value of the periods of visibility in the ergograph series. The numbers indicate fifths of seconds.

Compare the above Tables with the following one taken from Marbe's investigation.

TABLE III.
(*From Marbe*.¹)

SUBJECT.	DISTANCE FROM OBJ. IN CM.	AV. F.	AV. INTERMISSION PERIOD.	AV. SENSATION PERIOD.	INTER. INTO SENSATION.
G. M.	370	7.8 ²	2.7	5.1	1.9
G. M.	483	9.0	4.5	4.5	1.0
K. M.	215	11.9 ²	2.1	9.8	4.7
K. M.	425	17.4	10.2	7.2	0.7

¹ *Op. cit.* p. 623.

² The time here is in seconds.

What Marbe designs to show by the Table is that the efficient perception of the stimulus varies with the distance of the object. He found the same true with variations in the brightness of the object, and summed up his conclusions in the formula that the fluctuation lengths are a function of the intensity of the stimulus. A comparison of the two Tables will show that our results are in close agreement with his except that in the ergograph series the periods of visibility are also shortened. As to the main point, as shown in the last columns, there is substantial coincidence. The conclusion from this is that muscular effort has the same effect on the fluctuations as an increase in the intensity of the stimulus.

How is this to be explained? Without anticipating more general theoretical conclusions, it may be said that the probable explanation is the same as that of the increase in the efficiency of a motor activity by other motor activities. In other words, that impulses emanating from motor cells act upon sensory cells by way of increasing their excitation. It is probable that some of Münsterberg's results, *e. g.*, the shortened fluctuations with rapid breathing, and the shortened periods of non-visibility with eye movements and quick closing of the lids, can be explained on the same basis as being due to the exertion of effort. Another apparent effect was that the subject could see the fainter rings at the periphery of the disk during the dynamograph or ergograph series, which were invisible during the normal series. This cannot be stated positively as it might have involved an element of suggestion.

An additional fact that may be noted in this connection is that the increased efficiency shown in the ergograph series is the same as the lowered limen of sensibility under the condition of maximal attention. The close connection between maximal active attention and strain sensations resulting from motor activity, hardly needs to be indicated. The bearing of the general questions of distraction and apperception upon what has been said, will be noticed in a later connection.

3. RELATION TO VASO-MOTOR PERIODS.

The investigation to this point has been concerned with what seems to be merely extraneous influences upon the course of fluctuations. The question now arises as to how the fluctuations themselves are to be explained. Is the process that causes them different in kind from the processes that influence their variable lengths? To pass over now to the purely psychological view of apperception would be a violation of the common psychophysical assumption of parallelism. Our further experiments answer the above question in the negative.

At this time an investigation¹ was being carried on in the physiological laboratory by Professors Lombard and Pillsbury relative to the changes in the rate of beating of the normal human heart. Distinct periodic changes in the pulse rate were made out, which were found to stand in constant relation partly to the respiration and partly to the Traube-Hering waves. The only possible explanation seemed to be that various impulses notably from the vaso-constrictor center flow over and act upon the vagus center, the effect showing itself immediately in an inhibition of the control apparatus and consequent accelerated action of the heart.

These experiments called our attention to the approximate equivalence in time existing between the fluctuations and changes in heart-rate, and at the same time showed a type of nerve activity that might possibly be a means of explanation. Since the changes had, as indicated, been traced to vaso-motor activity, it was determined to try the fluctuation series in connection with the vaso-motor changes. For this purpose it was necessary to alter slightly the arrangement of the apparatus. The curves were registered on the vertical drum of an ordinary kymograph, driven by clockwork. The rate of revolution was adjusted so that the fifths of seconds indicated by the time-marker were just distinguishable. This slow movement required a sharply outlined registration of the beginning and end of each fluctuation, so, instead of the tambour and pressure-key, an electrical marker was used. An ordinary telegraph key served to make and break the current. The various changes in blood-pressure were registered by means of a delicate piston-recorder² connected with a finger-plethysmograph. The point of the piston-recorder was kept on an exact vertical line with those of the time-marker and the marker indicating the fluctuations. The plethysmograph was surrounded by a water-jacket through which a stream of water of constant temperature was passed. This arrangement served to register the pulse, the respiration periods, the Traube-Hering waves, and all other changes in volume. The record varied with the different subjects, and with the same subjects under different circumstances. At one time the pulse only would be distinguishable, at another time, the respiration or Traube-Hering waves, so that it was only under the most favorable circumstances that a record of all the changes could be secured at once. The pointers were placed on the same line, and a direct comparison of the parallel tracings was possible. Attention should be called to the fact that this method of exper-

¹ American Jour. of Phys., Vol. VII, p. 201.

² For a description of this instrument and its uses, see Amer. Jour. of Phys., Vol. VII, p. 186.

menting made it quite impossible for any predisposition on the part of the subject to influence the validity of the results.

The tracings reproduced in Figure 3 will illustrate the method and the result reached by it. The upper curve indicates time in seconds; the middle one, the series of fluctuations, the raised parts corresponding to the periods of visibility; the lower shows the changes in blood-pressure, only the pulse and Traube-Hering waves being distinguishable.

Fig. 3. On S.

The coincidence of the periods of visibility and those of increased blood-pressure in the finger is immediately evident. This was substantiated on subjects P. and S. by about fifty series on each. The reason for such an extensive experimental test is that it was exceedingly difficult to keep the circulatory conditions sufficiently uniform to obtain a continuous series of vaso-motor effects. The lever of the delicate piston-recorder was lowered with the slightest variation in conditions of sound or light, or the intrusion of any foreign idea into the subject's consciousness. In estimating the results, only those sections of the tracings showing distinct waves for five or more successive periods, were treated as evidence. The following method was observed in reading the tracings. The plethysmograph curve was levelled up to a straight line directly under the fluctuation curve, by means of dividers separated to the exact length of the piston-recorder lever. This made it possible to compare directly the two curves with a view to determining their coincidence. A case was treated as one of coincidence when the period of visibility began during the rise of one blood-pressure wave and ended before the beginning of the next. Every case in which the period of visibility began during the decline or reached over to the next, or in which there was a fluctuation without a corresponding blood-pressure wave or *vice versa*, was treated as a contrary case. The general estimation of the readings, made under the above restrictions, shows the following rate of coincidence as compared with all the cases:

For subject P., 83% in 106 cases.

For subject S., 90 $\frac{3}{4}$ % in 258 cases.

The presence of even these few contrary cases might throw doubt upon any conclusion based upon the results, but it must be remembered that there are many sources of interference quite beyond experimental control. There is always the possibility of a slight inaccuracy in the subject's registration of the fluctuations. Again, as we have already seen reason to believe, there are probably other physiological processes that exert an influence upon the length of the periods.

In the course of our experiments there appeared, in addition

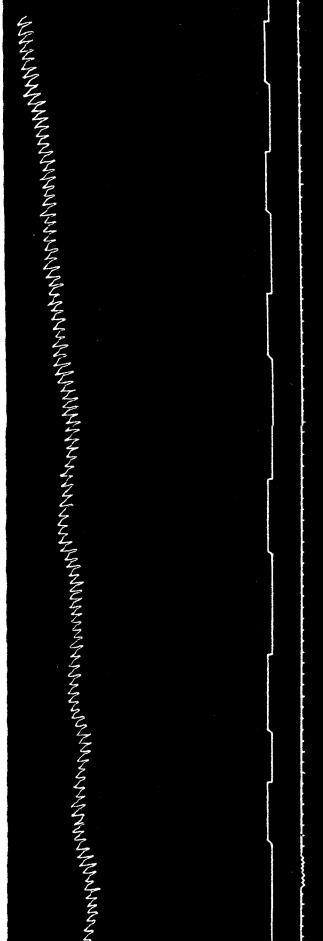


Fig. 3 on S.

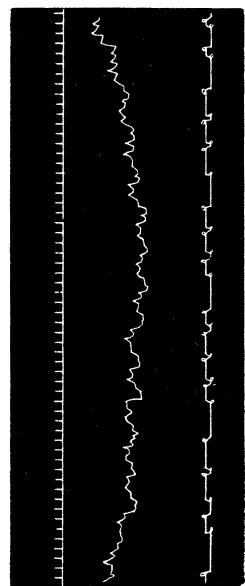


Fig. 4 on E.



Fig. 5 on P.

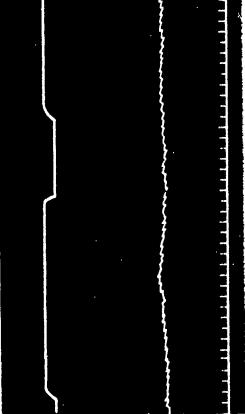


Fig. 3 on S.

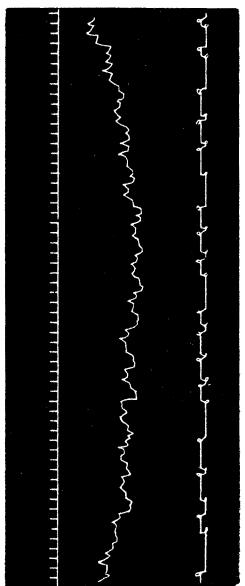


Fig. 5 on P.

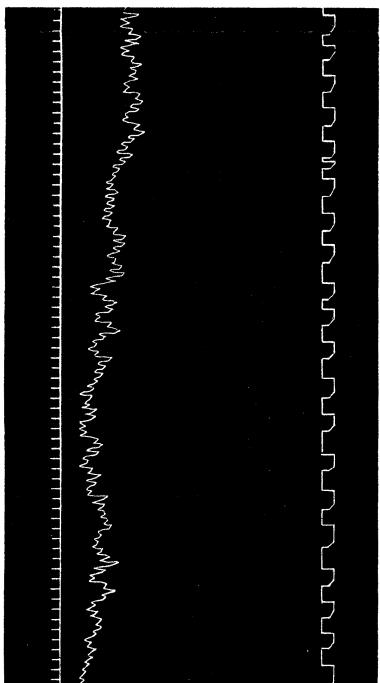


Fig. 6 on St.

to those mentioned, a long plethysmographic wave, the origin of which could not be accounted for. This came out only under the most favorable circumstances, and was very susceptible to any kind of disturbance. The measurement of these showed an average of 44 seconds, with an average variation of 4.25 sec. There is some reason for supposing that there is a connection between the variations in fluctuation lengths noted in Sec. 2 and these long waves, but the difficulty of obtaining a number of the latter in series made us waive for the time being the experimental test of the point.¹

The peculiarity of one of the subjects, *E.*, should be noted. From the first extraordinarily long fluctuations were given, the periods averaging 21 sec. with an average variation of 3.16 sec.

Fig. 4. On E.

The introspective record showed the reliability of the subject's judgments as to lengths. When later the plethysmograph was used, it seemed impossible to obtain even a pulse tracing. By increasing the temperature of the room and the water in the jacket, some of the waves were brought out. The pulse was never visible, the respiration wave never came out very clearly, and there was an exceedingly long vaso-motor wave, only slightly discernible. This latter coincided very closely with the long fluctuation mentioned.

The discussion of the theoretical bearings of this section will be taken up in the conclusion.

4. RELATION TO BREATHING.

The fact was noticed early by several of the subjects that if, during the period of disappearance, an especial effort was made to recover the stimulus, the rings would flash out for a moment and immediately disappear. This was undoubtedly the source of the indefiniteness and confusion experienced by the subjects during their preparation. The training directed their attention to the longer fluctuations, and gradually made the momentary flashes imperceptible. Near the close of the experimental work, *P.*, being the most thoroughly trained subject, began to

Fig. 5. On P.

look for these and register them together with the longer fluctuations. No cause for their existence could be ascertained until the pneumograph was tried, when they were found to

¹ Howell and Shields found similar long plethysmographic waves but of greater length in their work on sleep, but so far as the writer knows no physiological explanation has been offered for them. *Journal of Exp. Med.*, Vol. II, pp. 325-326.

follow accurately the periods of respiration. Then a subject was tried, who had been trained before but not used on account of the extreme shortness and constancy of his fluctuations. Only the plethysmograph was used, on account of the possible suggestion that might be given by the pneumograph. The result showed a strong pulse tracing and an extremely marked respiration wave. There was little or no indication of the Traube-Hering waves. The fluctuation periods corresponded closely with those of respiration. There were at in-

Fig. 6. On St.

tervals breaks in the correspondence, but this is what would be expected from the interference of the longer period. When the subject was questioned at the close of the work, he stated that he had been perfectly unconscious of the respiration process, that a connection between breathing and the fluctuations had never occurred to him.

The results here are in agreement with those of Lehmann who found in the case of minimal electrical stimulation an almost perfect correspondence between the fluctuation and respiration periods. The ambiguity in his method of explanation has already been indicated.

One important result to which this section brings us, is the complete uselessness of attempting to establish an absolute value for the fluctuations. The physiological influences that control them seem to differ widely with the various subjects and for the same subject at different times. This probably also accounts for the disparity in time values obtained from different sense departments.

The general results of our experiments would show, then, that the fluctuations of the attention are in close connection with at least three physiological rhythms. That in most subjects they run parallel to the Traube-Hering waves of blood pressure, as Exner has suggested might be possible.¹ That in some subjects the breathing undoubtedly plays the predominant role, and that in one subject it is probable that there is still another longer wave that assumes the more important place. Furthermore, even in the subjects whose main rhythm corresponds to the Traube-Hering waves the other influences are not entirely lacking. As Lehmann found, and his results are confirmed by the work of Taylor, even the longer attention waves tend to change their direction at a definite point in the respiration rhythm. Again, careful observation shows that the breathing rhythm continues to bring out the gray rings during the time occupied by the trough of the Traube-Hering wave.

¹Exner: *Entwurf zu einer physiologischen Erklärung der psychischen Erscheinungen*, p. 303.

On the other hand the longer waves, whatever their origin may be, betray their influence in the varying lengths of the fluctuations corresponding to the Traube-Hering waves.

The interaction of these varying influences, now one, now another predominating in the different individuals and in the same individual at different times, would account for the variations in length, the limits of which have been set by Pace as 3 and 24 seconds.

5. THE CASE OF O.

The present section has little connection with the above series of experiments, but should be appended as the description of an interesting case in connection with the old discussion as to the origin of the fluctuations. *O* was troubled by cataract in both eyes. In an operation taking place in July, 1890, the lens of the right eye was punctured, resulting in degeneration. The entire lens was removed in two subsequent operations. The same series of operations had been begun upon the left eye which, however, is not yet effective for seeing.

Owing to circumstances it was impossible to give much preliminary training, or take a very extensive series of experiments, so the reproduction of figures will have only a relative value. The averages found are:

Whole fluctuation, 6.72 sec.

Period of visibility, 4.89 sec.

Period of non-visibility, 1.83 sec.

The main point of interest is that the distinct appearance and disappearance of the gray rings took place just as with a person of normal vision. With the absence of the power both of convergence and accommodation, the result bears conclusively upon the peripheral theory of Münsterberg, and especially upon Heinrich's rather dogmatic support of it.¹ It again possesses a decided advantage over the results of Pace as he could never be absolutely certain that the atropin had entirely destroyed the power of accommodation.

III.

That nerve cells interact in some way, and that the activity of large groups of them can be focussed in definite directions, is an assumption that lies at the very basis of all psychological investigation. So long as no question as to how this activity goes on is raised, the assumption is granted without contradiction. The existence of the suitable anatomical structures is sufficient evidence that it takes place. One definitely proved physiological fact would go far toward settling psychological

¹ Die moderne physiologische Psychologie in Deutschland, pp. 125, ff.

disputes as to reinforcement and inhibition. There are, to be sure, a few special kinds of nerve activity which are intelligible to the physiologist. For example, (a) the transmission of sensory and motor impulses from one neuron to another; (b) the inhibitory control of certain reflexes such as those of micturition, parturition and defecation; (c) the flowing over of impulses from a sensory to a motor center, such as the fact discovered by Schäfer that stimulation of parts of the occipital and temporal cortex is followed by definite movements of the eye.¹ Investigations throwing light upon the influence of psychical states upon nervous activity are scarce but not entirely wanting. For example, Lombard has shown that the knee-jerk is reinforced by attention, mental work, music, etc.²

Our own investigation of the vaso-motor processes and voluntary effort, cannot be said to prove conclusively the additional fact that the activity of motor cells reinforces the activity of sensory cells, because one of our terms was a psychical state probably involving a number of factors, but the connection between this state, primarily bound up with a sensory process, and the motor activities mentioned is unmistakable. It might be argued regarding the connection with the vaso-motor process that the increased efficiency of the attention during the period of visibility is due to the increase in blood supply. This is the position of Lehmann, in the way of which there are certain very definite difficulties. In the first place, Lehmann takes as the basis of his theory the fact that when any organ is active it receives a larger supply of blood. From this he immediately concludes that the rhythmical respiration process alters the supply of blood to the brain. All that can be concluded from the premise is that the alteration is in the supply of blood to the respiratory organs. In the interval it may go to the brain or may not. Again, on his basis the impulse which serves as a demand for increased supply must originate in the activity of the organ itself. Consequently the periodic presence of a larger amount in the brain assumes a *prior* periodic increase in brain function, which puts the problem further back. In the third place, without attempting to dogmatize on the matter, it is almost safe to say that the rhythmical Traube-Hering waves, with which we found the fluctuations connected, are not the result of a specific demand from any part of the body. When such demand is made, the registration of these waves is interfered with, as was seen in our experiments in the instant fall of the lever when effort of any kind on the part of the subject took place.

¹ Proceedings of the Royal Society, 1888, Vol. XLVIII.

² Amer. Jour. of Psy., I, 1.

One other question stands in the way of our making a definite affirmation in regard to vaso-motor activity, viz., the unsettled difference between the positions of Mosso¹ and Howell concerning contrary circulation. If the supply of blood in the brain is the reverse of that in the body, then obviously in our case some cause for the fluctuations other than the presence of blood must be looked for. On the other hand, if the circulation is direct in both body and brain, the blood supply factor may enter in. But granted that the latter view is correct, the question still remains as to whether the presence of more blood has any effect whatever upon the function of nerve cells.

The phenomenon seems most explicable when put on the same basis as the fact that respiration waves are found in blood-pressure tracings, and that both respiration and vaso-motor effects are found in the rate of the heart. That is, the two centers, controlling functions of enormous importance and extent, must originate correspondingly powerful impulses, and these impulses not only proceed along their regular paths but overflow to other centers and are transmitted to the cortex, there acting by way of added excitation upon groups of sensory cells. The question might be raised as to whether, granting an influence of some kind, the impulses reinforce the process of stimulation or add to the energy of the cell. The question, however, is not important, for us the significant point being that *they in some way reinforce the functional activity of sensory cells*. We have already indicated the fact that sensory stimulation has the same effect upon the fluctuations as voluntary effort. So our general conclusion would be that the activity of any group of cells is not only a direct response to the stimulus, but is in large measure dependent for its efficiency upon the reinforcing influence of other groups of cells.

An objection to this position may be based upon the question of distraction. According to the Wundtian position, distraction, as seen in the apperceptive struggle of ideas, lies at the basis of the fluctuations. Eckener, as the exponent of this position, gives a long list of the possible influences claiming attention. Prominent among these are mentioned the sensations arising from the general stimulation of the sense organs disregarded for the time being. The objection is answered by the investigations of Miss Hamlin² and Münsterberg³ who showed that these influences, so far from being detrimental to the activity of the attention, are really necessary to its highest efficiency.

These results are in entire accordance with the type of ner-

¹ Ueber den Kreislauf des Blutes im menschlichen Gehirn, 1881.

² Amer. Jour. of Psy., VIII, p. 1.

³ Psy. Rev., I, pp. 39, ff.

vous activity to which our experiments on the whole seem to point. They further seem to furnish the best method by which to approach the general question of apperception and the mode of its activity. The view which regards the apperceptive relations of ideas as based entirely upon inhibition, is founded upon a kind of mental mechanics not at all consonant with the facts of nerve physiology. Inhibition is certainly one of the functions of the nervous system, but the word is used with reference to the effect and not the process. Nor is the position in agreement with the results of the investigations of Münsterberg and Miss Hamlin just mentioned. We naturally deal with the question from the point of view of our own experiments. While our results are not sufficient to warrant the final affirmation of a theory of reinforcement, still the facts regarding nerve function, the basis most needed for any kind of theory, point decidedly in that direction. The affirmation of the importance of this side has been definitely made by Exner and others of prominence. The view makes most intelligible the fact of unified interactivity of conscious elements, and at the same time saves us from the difficulties incident to regarding the attention as a special process.

SUMMARY.

The results of the investigation, stated generally, are:

1. The fluctuations of the attention do not depend, as formerly affirmed, upon either the apperceptive process or changes in the sense organ.
2. The periods do not remain constant but have a definite order of variation.
3. Voluntary effort shortens the fluctuations and increases the relative efficiency of the attention.
4. The periods stand in close relation with the vaso-motor and respiration processes.
5. The causal process is physiological in nature, and probably acts by way of a reinforcement of the activity of the nerve cell, not indirectly through changes in nourishment, due to variations in blood pressure.